Professor of Mineralogy] I have to go to London two days every fortnight as President of the Geological Society, and am printing a book which I have not yet written [" The History of the Inductive Sciences"], so that I am obliged often to run as fast as I can to avoid the printers riding over me, so close are they at my heels. I am, in addition to this, preaching a course of sermons before the University; but this last employment, though it takes time and thought, rather sobers and harmonises my other occupations than adds anything to my distraction." He seemed to be able to turn his hand to anything, and, like a dexterous conjuror, play with half-a-dozen balls at once. Pendulum experiments, theories of the tides, mathematical problems, crystallographic formulæ metaphysics, and various subjects in moral philosophy, classics, modern languages, architecture, geology, with plenty of work in general literature, all make up what we may call in the best sense "farrago libelli." These letters also bring out very clearly another characteristic of Dr. Whewell's mind. He was essentially cautious in regard to change—an advocate of reformation rather than of renovation; in science a systematiser rather than a discoverer; like a navigator who explored to the full the uninvestigated coasts of the Old World, rather than one who steered out into the open ocean in the hope of discovering a New World. This was no doubt partly due to his mathematical training and academic habits of life—but it is very rare, perhaps impossible, to find a memory of extraordinary tenacity and a life essentially studious, combined with originality in one of its highest forms. That requires a good deal of mental fresh air, and is apt to droop a little if too much confined to the atmosphere of a library. This is especially evident in Dr. Whewell's remarks upon the "Vestiges of Creation" and in his essay on the "Plurality of Worlds." The same tone of mind is very conspicuous in his attitude towards the question of University Reform. He was a vigorous opponent of the abuses of private tuition, a zealous advocate of progress in every department of learning, deeply anxious for the improvement of the Classical and Mathematical Tripos examinations, and to him more than to any other single man the recognition of the Natural and Moral Sciences as branches of academic study is due. But he was antagonistic—almost bitterly so—to the appointment of the Royal Commission of 1856 and of its successor, and was hostile in many respects to the changes—now almost universally acknowledged to have been on the whole very beneficial—which were introduced by the statutes of 1859–61. His great hope and desire was that the University should be allowed to reform itself, and be spared any interference from without. That he should have entertained this hope after so many years of academic labour is perhaps the strongest proof of his sanguine temperament.

We must now part from this interesting volume. Perhaps—like the portrait prefixed to it—it slightly fails in depicting the characteristic ruggedness of the man, but it does much to show him as he was to those near and dear to him as well as to the world—a man of immense intellectual power, of intense energy and industry, of high purpose and simple piety, a hard hitter in conflict and a lover of the shout of battle, but too magnanimous to bear ill-will, whether in defeat or victory. Besides this he was a munificent benefactor to his College and his University: one to whom both must long be grateful, and of whom both may well be proud, as having filled a great position in the world of science and literature, and especially as being "the man" (in the opinion of a most competent judge) "to whom, more than to any other single man, the revival of philosophy in Cambridge is due." T. G. BONNEY

OUR BOOK SHELF

Inorganic Chemistry. Adapted for Students in the Elementary Classes of the Science and Art Department. By Dr. W. B. Kemshead, F.R.A.S., F.G.S. Enlarged edition, revised and extended. (London and Glasgow: William Collins, Sons, and Co., Limited.)

THIS work is a typical one. While containing much that is useful and fairly satisfactory, especially from an examination point of view, the whole tendency of the book, considered as an elementary treatise on a branch of natural science, must be strongly condemned.

The leading facts concerning the better-known non-metallic elements and compounds are succinctly stated; the principal reactions of formation and decomposition of these bodies—especially those reactions which unfortunately *must* be "got up" for examination purposes—are arranged in the form of equations; and the simpler arithmetical applications of such equations are illustrated by fully worked-out examples. But chemistry is more than this: facts must be connected together by principles; the connection between fact and theory, and between theory and fact, must be revealed; these two must not be regarded as synonymous, but as mutually dependent; and the reasoning by aid of which theoretical conclusions are reached must be clearly indicated. Chemistry is neither a system of dogmatic assertions nor an accumulation of shibboleths, by the skilful use of which an examiner may make havoc among the Ephraimites crowding to the Jordan of Examination, but a living science.

The principle which is most largely used (or rather misused) in Dr. Kemshead's book is that of Valency; but valency in the hands of this author is deprived of its value as a scientific theory, and becomes an accumulation of fanciful speculations. The basis of the present work is evidently Dr. Frankland's "Lecture Notes"; hence probably the success of the book in preparing examinees for South Kensington (the present is a second and enlarged edition); and is not such success after all of more importance than training chemists or disciplining the mental powers of youth?

The theory of valency is based on the wider molecular theory of matter, which was preceded by the atomic theory of Dalton, itself a development from that system of chemical notation which rested on the combining weights of the elementary bodies. Now it is clear, from many passages, that the author of this book has failed to distinguish combining weights from atomic weights, and atomic from molecular weights: thus on p. 13 we read "these proportions by weight [*i.e.* from the context, these proportions in which "substances unite together chemically"] when reduced to their lowest relative value, and expressed with reference to that of hydrogen, which is usually taken as unity, are called the atomic weights, or combining numbers of the elements." Again, on p. 26, "the *combining weight* of hydrogen being 1, that of oxygen becomes 16; of nitrogen, 14; of carbon, 12," &c. But combining weights are *not* synonymous with atomic weights, and the *combining weight* of oxygen happens to be 8, of carbon 3, and of nitrogen 4·66. The formula weights of compounds are constantly referred to as "atomic and molecular weights." We have such formulæ as $(NH_4O)_2$, CuO_2, &c., stated to be molecular formulæ;

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molecule is nowhere defined (in a note on p. 57 a casual statement is made as to the meaning of the term); "Avogadro's law," which lies at the basis of the whole modern edifice of chemistry, is conspicuous by its absence; certain statements as to gaseous combination and to "volume weights" are made, it is true (p. 35), but these are incomplete and misleading.

When a theory of valency is raised on so slender and shifting a molecular foundation as is here laid, no wonder that the edifice should be a strange one; the definition of "atomicity" on pp. 54-55 is incomplete, and cannot be upheld by facts; the statement on p. 58, "it is then a law to which there are no real exceptions, that though the equivalence of an element may vary, it does so always by the addition or subtraction of an even number," is simply untrue. As an "important conclusion" from certain "facts" (? fancies) "on equivalence," it is stated that (p. 59) "a formula which possesses an uneven number of bonds or units of chemical affinity cannot possibly represent a molecule"; without minutely criticising the expression "bond or unit of chemical affinity," suffice it to say that such a formula as, according to Dr. Kemshead, cannot possibly represent a molecule, unfortunately does represent a molecule. The existence of the molecule NO is a case in point: *à propos* of this compound, there is a charming example of the author's method of treating chemical science as a collection of opinions of various authorities to be found in a footnote on p. 169.

Notes on the Crania of New England Indians. By Lucien Carr. From the Anniversary Memoirs of the Boston Society of Natural History, 1880.

THIS is one of the numerous contributions now being made towards our knowledge of the fast-disappearing race of North American Indians. The author, Mr. Lucien Carr, holds the office of Assistant Curator to the valuable Museum of American Archæology and Ethnology at Cambridge, Mass., an institution owing its foundation to the liberality of Mr. Peabody, so well known in England by his benefactions to the London poor, and its scientific excellence to the zeal and organising power of its first curator, the late Dr. Jeffries Wyman, and of his successors.

The object of the present memoir is to collect together such information as is still to be obtained regarding the cranial characters of the native Indians of the New England States, the celebrated "five nations" of the early historians of America, who in consequence of their geographical position were among the first of the race to succumb to the inroads of European immigration. Measurements are given of 67 crania, of which 38 are assigned to males and 29 to females. The averages of these measurements give the following results:—A medium cranial capacity, *i.e.* 1436 cubic centimetres for the males and 1319 for the females. A latitudinal index of ·759, showing mesaticephalism verging upon dolichocephalism. The altitudinal index exactly the same. The principal facial indices show orthognathism, with a strong tendency to mesognathism, a mesorhine nose (index 50), and slightly megaseme orbits (index 88 in the males, and 91 in the females). Although these are the average characters of the whole collection, very few, if any, of the individual crania are to be found presenting them. There is indeed no such uniformity among these skulls as may be seen in certain races, such as Eskimos, Bushmen, Fijians, Andamanese, or even Australians. Perhaps it could scarcely be expected in inhabitants of a large continent, presenting great diversities of climatic and other conditions, and with no natural barriers to free migration and intercourse. The examination of these skulls therefore confirms what has been often remarked before, that although in a broad sense the American Indians present a certain community of type, there is great diversity in detail among them, the result probably of a long series of repetitions of the process of breaking up into distinct groups or tribes and reuniting in various combinations.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Conservation of Electricity

IN a recent communication to NATURE (vol. xxiv. p. 78) Prof. Silvanus P. Thompson very kindly mentions my latest memoir on "The Conservation of Electricity," and, as I am glad to find, confirms my views on this subject by stating that he has independently arrived at the same conclusions with myself.

As regards however the question of priority moved by Prof. Thompson, I think I ought to add that an earlier paper of mine, published five years ago, must have escaped Prof. Thompson's attention. This was printed as an abstract in the *Comptes rendus* of the Paris Academy of Sciences for June 19, 1876, under the title, "Extension du principe de Carnot à la théorie des phénomènes électriques. Équations différentielles générales de l'équilibre et du mouvement d'un système électrique réversible quelconque." I there enunciated the law of the Conservation of Electricity in the same terms as now, and also gave the same analytical method for applying it. I beg leave to quote as a proof the following explicit passage from this extract:—"L'équation $\int dm = 0$ a une autre signification plus simple; elle signifie que de l'électricité peut se déplacer, mais ne peut jamais varier en quantité. Ce principe de la conservation de la quantité d'électricité à été admis par les physiciens dans tous les cas connus jusqu'ici, influence, frottement, etc. . . . . Pour que $\int dm = 0$ pour tout cycle fermé, il faut que dm soit une différentielle parfaite." This method I had already applied in 1875 to the phenomena presented by mercury electrodes (*vide Annales de Chim. Phys.* 1875). In fact my latest memoir is merely a renewed attempt to draw, by means of new applications, the attention of physicists to a fact which I cannot help considering as important for the future, viz. that the principle of the Conservation of Electricity is, *as far as analytical applications are concerned*, the exact analogue to Carnot's Principle for Heat.

Paris, Faculté des Sciences, June 5 G. LIPPMANN

Apparent Decomposition of Sunlight by Intermittent Reflecting Surfaces

IT occurred to me that light might be decomposed by interrupting, with a reflecting surface, a ray of light in such a manner that the interruptions may be proportional to the wave-length period of any particular ray forming a part of a composite ray. The experiment is effected in the following way:—

A wheel having bright spokes (the large wheel of a bicycle answers well) is caused to revolve between an observer and the sun, so that a ray of light is reflected to the observer by a bright spoke; then, when 120 spokes pass before the observer per second, violet light shines out vividly; when 65 pass, red appears, and different rates of revolution give different colours. There seems to be a marked relationship existing between the number of spokes which pass by and the wave-length of the two colours mentioned, that of the violet being $\frac{1}{60000}$ inch, and that of the red $\frac{1}{34000}$ inch.

I am now investigating this apparent relationship between spoke-interruption and wave-length for the other colours of the spectrum of white light, and I hope to be able to make known the results shortly. FREDERICK J. SMITH

Taunton, June 4

Symbolical Logic

I AM sorry that Mr. MacColl should have thought that there was any intention on my part to suggest a doubt as to his having

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